Through the past labours of these men and of their chief officers, the business of the meteorological office has now probably been put into a satisfactory position that will render unnecessary for the future any very great expenditure of energy upon the details of administration.

But such an arrangement, however excellent in a business point of view, must nevertheless necessarily fall short in developing scientific research. For this the undivided attention of several men of science must be secured, and the question we would here wish to submit to the consideration of our readers is the following.

Would not the combination of a few such men devoting their whole time to the subject, together with other men who though well acquainted with the subject, and otherwise qualified, are yet unable to devote their whole time to it, constitute the best possible committee of the future? We need hardly say that the functions of such a board would not be limited to that of producing research within itself. It ought likewise to stimulate and aid outsiders by various means, including advice and perhaps pecuniary aid. It might attach to itself as occasional members the meteorologists of the provinces, inviting their co-operation, giving and receiving advice, and it might even associate with itself as corresponding members, the meteorologists of the colonies and of foreign countries. In fine, the subject is one which perhaps more than any other demands the united action of men of various nations.

From what has just been said, it will readily appear that the sources of information upon which such a committee will draw in their investigations will by no means be confined to those which are under their own immediate control. The stores accumulated by foreign and colonial observers will, of course, be greatly drawn upon, and not only so, but the committee will doubtless also avail themselves of the stores of information possessed by other Governmental departments, as, for instance, those under the control of the hydrographer, who would naturally be a prominent member of the meteorological board, lending them his valuable assistance and co-operation. Besides the hydrographer, it would probably be found necessary to have, at least, three members of the board representing the three divisions of the subject already alluded to, who should be content to devote their whole time to their respective inquiries. The remainder would be composed of distinguished men interested in the subject, but unable to devote their whole time to it, embracing amongst them one or more mathematical physicists of high reputation.

If it be asserted that there are difficulties in the way of such an arrangement, it may be replied that undoubtedly there are; but if the subject were not one of difficulty, the Government would probably not have consulted the Royal Society from the very commencement of their inquiries. Such a powerful engine as a distinguished scientific committee, some of whom are pledged to devote their whole time, and others a portion of it, to the progress of scientific meteorology, is not meant to be used for the mere chopping of straws. The appropriate function of such a committee is surely that of overcoming difficulties.

BALFOUR STEWART

THE "ENCYCLOPÆDIA BRITANNICA."

Encyclopædia Britannica. Vol. IV. (Edinburgh: Adam and Charles Black.)

THE most prominent scientific contribution to the fourth volume of the "Encylopædia Britannica" is Prof. Balfour's article on Botany. In fact, with two other articles, it occupies a fourth of the whole number of pages, and this, together with its very comprehensive title, leads the reader to expect a tolerably complete review of all the various fields of botanical science, an expectation which is confirmed by their enumeration in the opening paragraphs. A little further examination shows, however, that it only treats of a single branch; the "Structure and Morphology of Plants;" "Classification" and "Distribution in Time and Space" are deferred for separate articles, and "Vegetable Physiology" has apparently dropped out of sight altogether. Any division of the matter is, for many reasons, better in an Encyclopædia than to despatch a whole subject *en bloc* with what is substantially a textbook rather than an article. But it is impossible not to regret that the vegetable side of Biology has not had a carefully planned series of contributions by different hands devoted to it like those which from the volumes already published appear to have been arranged for the animal side. And it is at any rate not easy to see why, as it is, one of several contributions should arrogate to itself the general title belonging to the whole. What would be thought of an article headed Zoology which only dealt with the myology of mammals?

The "Encyclopædia Britannica" has become, in its present edition, in a sense a national undertaking. It is so well supported by the best men in different departments of knowledge that it will no doubt come to be regarded as having a kind of representative character. The utterances of the several contributors will be taken as a kind of measure of the state of opinion in this country in each subject. From this point of view it is impossible not to feel that Prof. Balfour's exposition is disappointing as coming from so eminent a teacher, and that the idea it gives of botanical science is unsuggestive to the last degree.

Passing over an historical sketch of which many of the details, such as the last illness of the elder De Candolle, are essentially biographical, we commence with the "Structural Elements of Plants," in other words, their "General Histology." This opens with an account of the cell, which, even in its youngest condition, is stated to contain a sap-cavity; this is by no means the case, and the adjoining illustration, to which reference is made, shows cells with unvacuolated protoplasm, unless the nucleus is made to do duty for a vacuole. On the next page we are told that protoplasm "consists of albuminous substance mixed with water, and some incombustible materials," and that "it also contains some organic compounds;" are we to infer from this that albuminous substance is inorganic? From the cell we pass to the consideration of tissues, which are divided into cellular and vascular. This distinction carries us back half a century to De Candolle's "Organographie Végétale" (1827). Vegetable histologists have, indeed, laboured in vain from

Von Mohl onwards, if they have not succeeded in showing that *all* plant tissues are cellular. Coming to details, it is a little surprising to find (p. 87) sieve tubes (*siebrohren*) confused with dotted ducts (*porengefässe*) ; the two structures are entirely distinct and are characteristic respectively of the "bast" and "wood" tracts of the fibro-vascular bundle. It is remarked that the "latticed cells of some authors are of a similar nature ;" but the real fact is that they are the same thing. Hartig, who discovered the sieve-tubes in *Cucurbita,* found the pores open and occupied by threads of protoplasm which united that of adjoining cells ; Von Mohl, doubting this, proposed for them the name of *gitterzellen,* so as to avoid the implication of perforation. Under the head of substances found in cells, we have, of course, an account of starch, sugar, gum, &c. The very important fact that starch grains are always formed in protoplasm, which is also the agent in their eventual solution, is overlooked. Gum, also, is not usually found in cells at all ; it is the result of a mucilaginous change of the cell-*walls.*

Next in order would come an account of the principal systems of tissues—epidermal, fundamental, and fibro-vascular. Prof. Balfour treats of the former alone, only recognising a classification of the tissues of the plant into an outer layer bounding an inner mass. This leads him into numerous difficulties. Thus he calls the velamen of the aerial roots of orchids (which is a development of the epidermis), hypoderma, although *that* is a modification of the fundamental *i.e.*, as the name indicates, sub-epidermal tissue. Again, he derives cork from the epidermal layer, whereas it is almost always developed from cortical tissue *under* the epidermis, which is usually destroyed at the time of its production ; lenticels, also are connected with cork-formation, and not, as Prof. Balfour states, with the development of aerial roots. It may be noted, also, that the popular theory of the action of stomata still keeps its place here, although long ago shown to be as often as not untrue.

The remainder of the article deals with the morphology of flowering plants with brief and utterly insufficient references to other groups lumped together under the general head of Cryptogams. Prof. Balfour is equally fond of the classification of plants into Dicotyledons, Monocotyledons, and Acotyledons, of which the value at the present time may be estimated by the fact that it was proposed by Jussieu in 1789, and is about as significant now as a classification of religions would be into Unitarians, Christians, and Heathen. Neither here nor elsewhere is there any grasp of general principles to relieve the arid monotony of technicalities often unfortunately inaccurate. A few instances of this may be noted :—The rhizome of Solomon's seal (p. 98) is a typical instance of a definite (*sympodium*), not of an indefinite rhizome (*monopodium*) ; the stem of the Shola plant is not wholly composed of pith (p. 100), but of a peculiar kind of wood ; the dark bands in the transverse section of the tree-fern (Fig. 89) are not woody fibres, they do not belong to the fibro-vascular, but to the fundamental system ; on p. 92 we are told that the group of Thallogens comprise Algæ and Fungi, while on p. 107 we find added "and many Hepaticæ"—nothing more being really meant than that they have a thalloid habit ; lastly, not to prolong the citations interminably, we are told on p. 119 that "the absorption of carbonic acid water and other fluids is carried on by the leaves, chiefly by the stomata," whereas it is pretty generally believed that the business of leaves is to get rid of water, not to take it in.

One feature of Prof. Balfour's article cannot fail to strike even the most casual reader. This is the extraordinary profusion of technical terms made still more repellent by catching the eye, in italics. Here are some specimens :—

"The names of *bothrenchyma* and *taphrenchyma* have been given to a tissue composed of such cells. Not unfrequently contractions are visible on the outside of the vessel, indicating its formation by coalescence of superposed cells. To vessels exhibiting contractions of this kind, whether spiral or pitted, the terms *moniliform* or *vermiform* have been applied ; and the tissue composed of these *moniliform* vessels has been denominated *phleboidal,*" p. 87.

"The *parenchyma* of the leaf is the cellular tissue surrounding the vessels, and inclosed within the epidermis. It has sometimes received the names of *diachyma, mesophyllum,* and *diploe,*" p. 108.

Now it will seem almost incredible to say that all but one of the terms italicised in the passages just quoted are absolutely obsolete. Science is like nature ; each stage in the progress of either is furnished with appropriate belongings and surroundings, which pass away for the most part, and give place to new. Literature preserves their remembrance in the one case as the safe keeping of the rocks does in the other. Science has its fossils as geology has, and if we delve after them into the *débris* of the past, it is to learn that the history of either has been continuous, and not to inform ourselves of the actualities of the present. But Prof. Balfour's article is like a breccia to which deposits of every age have sent ill-assorted contributions, and the waifs and strays of to-day have to settle down as best they can with the most ancient and singular remains. No botanist who is engaged in solving actual botanical problems—the existence of which readers of Prof. Balfour's article will hardly suspect—talks about *phleboidal bothrenchyma* or *diplöe* any more than he thinks of calling a walnut a *Tryma,* or a grape a *Nuculanium.* Students bent on passing examinations may perhaps persuade themselves, if not their examiners, that in committing such hard words to memory they are making solid additions to their mental furniture. But the real fact is that such things are of no use to anybody, and Mr. Bentham, than whom no living botanist has written more systematic works, has included all the terms—mostly extremely simple—with their definitions which are really needed for the purpose of describing plants, in a short pamphlet of some thirty pages. So that while Prof. Balfour has not succeeded in giving us—what it is the business of an Encyclopædia article to do—a comprehensive view of the broad facts of Vegetable Histology and Morphology in their present aspect, he has certainly not given an account of their terminology which does the subjects justice, but has produced a sort of terminological cemetery in which all kinds of decaying language have been affectionately embalmed.

The unfortunate result of this kind of treatment of the subject—and it is but fair to admit that Prof. Balfour is by no means the only culprit—is that, even amongst scientific men, botany has come to be regarded in this country as scarcely a serious branch of science at all, and as little more than a kind of biological equivalent of the "Use of the Globes," suitable, indeed, for ladies' schools, and useful, also, like "Materia Medica," for the purpose of occupying the attention of medical students so as to keep them out of mischief during their first summer session.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*]

Visual Phenomena

In support of Mr. Arnulph Mallock's conclusion (vol. xiv. p. 350) as to the cause of the star-shaped appearance presented to the naked eye by a bright point, perhaps the following epitome of some notes which I made last January may have interest:—

1. Looking at a distant lamp, with both eyes, I see a radiant corona round the lamp.

2. I find that this corona is composed of two coronæ superposed, one due to the right eye, the other to the left.

3. Each corona has distinctive features of its own, which are recognised in every observation and have remained the same for years.

4. The radiant beams (which are the conspicuous feature) are not exactly radial, but are forked once or twice.

5. The corona is bounded by a peripheral fringe of blue succeeding to red.

6. The diameter of the corona varies with the diameter of the pupil. The distal portions of the radiant beams are concealed or revealed by the contraction or dilatation of the pupil.

7. Any given part of the radiance may be cut off by advancing an opaque body in front of the eye, from the *same* side as the given part of the radiance.

An obstacle which cuts off only the central rays of the entering pencil only dims the central image, but does not affect the radiance.

The radiance, therefore, is due to the outside rays of the entering pencil becoming excessively refracted so as to be thrown across the visual line before reaching the retina.

8. There is nothing in the *cornea*, *aqueous humour*, or pupillary margin of the *iris*, that can cause such refraction.

9. It appears most likely that this excessive refraction of the outside rays of the entering pencil is caused by something in the crystalline lens—probably, in the first place, by undue convexity of the more marginal parts of the lens, which are uncovered by dilatation of the pupil.

10. The *radiant* appearance of the corona is probably due to the radiate structure of the crystalline lens.

The agreement between them is pretty close, extending to the furcate character of the rays.

11. On examining my own eyes by a pencil of diverging rays (admitted so as to throw a shadow of anything inside the eye upon the retina) I find that the main beams of the radiance correspond pretty well to the main radii of my lenses, in the opposite direction.

This confirms that the radiance is due to the radiate structure of the lens.

In one particular my observations differ from Mr. Mallock's. He finds that the length of the rays depends "on the brightness of the point" as well as on the size of the pupil. I find that points of different degrees of brightness have rays of the same length, that length being limited only by the size of the pupil, but that the *breadth* of each ray varies with the apparent size of the lamp-flame or other luminous point under observation. Indeed, if the lamp-flame is so bright as to stimulate the iris to contraction, the rays become shortened in like measure.

The radiate structure of the lens of the human eye is well shown in Kölliker's "Manual of Human Microscopic Anatomy" (ed. 1860, p. 568). The "non-fibrillated" or "central planes" there described are possibly of greater refractive power than the wedge-shaped fibrillated portions between them. This would cause the phenomenon of radiance by excessive refraction of the outside rays of the entering pencil.

Hubert Airy

Edensor, Kidbrook Grove, Blackheath, Aug. 28

Species and Varieties

It may be taken, I presume, that the description and naming of "species" has now a great value as material for studying the laws of the evolution of species and of geographichal distribution and variation; and that the question is not so much to know what name to call a "species" as to account for its presence and form in the economy of nature. And it will, perhaps, also be granted that the study of geographical distribution does not consist alone in acquiring a knowledge of the fauna of a district, so much as in investigating the laws of the special differentiation of that fauna. It thus becomes evident that the slightest modification tending to persistency requires the most careful record, as it is only by the knowledge of first slight modifications that we can expect to understand the process of larger divergencies.

In faunistic catalogues, should the author be inclined to the "lumping method" (perhaps in one sense correctly), a form may be considered as only a variety of some well-known species, and recorded under that name. We thus learn nothing of its modification, and are led to think of it as agreeing with the typical form, thus losing one of the most important facts in our study of variation and distribution. Another element of error seems also co-existent. When a newly-discovered form is designated a variety of another species, it would lead us to suppose that that species is the original type from which the other form has branched. But it is quite possible to suppose on such reasoning that the newly-discovered form may have been the parent from which the previously described species may have been derived. The offspring may have been collected and described before the parent; or we may be filling a space with planets to which there is no sun. This I have frequently suspected in the study of exotic lepidoptera, a study which is principally confined to the perfect form of the insects and of whose larval changes we know in most cases nothing. It rests generally on the judgment of individual entomologists as to the amount of difference to be considered specific.

Without venturing on the vexed question as to species and varieties, would it not be furthering the cause of science that what are considered as merely local varieties should, *as such*, have as careful a description as though they were ranked as species.

W. L. Distant

Buccleuch Road, West Dulwich.

Antedated Books

I cannot allow that our *Transactions* are antedated, as is asserted by "Another F.Z.S." As is the case with the *Philosophical Transactions* of the Royal Society and the Linnean *Transactions*, no date beyond the year of issue is given on the cover, but there is the additional advantage of the date at which each number goes through the press at the bottom of the sheets. There can, however, I think, be no objection to giving a still more exact date of publication on the cover, and this, in accordance with "Another F.Z.S.'s" suggestion, for which I am much obliged to him, I propose to do in future.

P. L. Sclater

I am sorry to see that "F.Z.S." still prefers to write under the signature of his first letter. I was in hopes that when he saw the gravity of the accusation which he was bringing against me, he would in his own person have disclaimed any intention of seriously laying such a charge to my door, but as no word of apology escapes him, I regret that in making such an injurious statement he has not added the weight of his name to the accusation. I am, therefore, at liberty to suppose that he is no stranger himself to the "evil practice" (*honi soit qui mal y pense*) which he deprecates, otherwise I cannot imagine such a thought occurring to anyone; but it is evident that on receiving the book his only impulse was to write a letter to this journal instead of charitably endeavouring to discover some feasible explanation of what he calls the "false" date. As most naturalists in England are in the habit of following the rules of the British Association in regard to nomenclature, "F.Z.S." can find more profitable